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What's behind pro-poor growth? An investigation of its drivers and dynamics

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Abstract
Standard growth incidence curves describe how growth episodes impact on the overall income distribution. However, measuring the pro-poorness of the growth process is complex due to measurement errors, and to the effect of shocks that may hit the percentiles of the income distribution in different ways. Therefore, standard growth incidence curves may misrepresent the true growth process and its distributive impact. Relying on a non-anonymous approach, we compare actual growth episodes at each percentile of the initial personalized distribution with counterfactual mobility profiles which rule out the presence of shocks. We consider Indonesia in 2000–2007 and 2007–2014, two growth spells in which there was substantial, significant upward mobility among the initially poorer, a sizeable part of which cannot be explained by unobserved individual endowments or standard socio-economic attributes. The difference between actual and expected growth is related, in the early 2000s, to the economy-wide transformations, which characterized the early years of the post-Suharto era. However, in the more recent years, it can be largely attributed to individual recovery from previous negative losses and high vulnerability and reactivity to shocks for the poor.

Keywords Indonesia · Shocks · Pro-poorness · Mobility

JEL Classification D31 · I3 · O12

1 Introduction

In the context of the Sustainable Development Goals, to promote countries' development and to raise the living standard of people at the bottom of the income distribution, the World Bank Group renewed its strategy by defining two goals: *(i)* ending the share of people living in extreme and chronic poverty by 2030; and *(ii)* promoting shared prosperity

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(Basu 2013). The first goal deals with the reduction to less than 3 percent of the share of people living below the World Bank's poverty line of US $1.25 per day. Empirical evidence has documented that economic growth represents the main tool to achieve absolute poverty reduction (Dollar and Kraay 2002; Dollar et al. 2016).

However, Basu (2013) has argued that the observed growth rates are not enough to eradicate poverty and a more equal distribution of growth benefits is desirable. This is the spirit of the "shared prosperity" goal, which calls for greater income growth of the poorest 40 percent of people. With the definition of these two goals, the World Bank Group recognizes that growth not only should be good for the poor but also has to be "pro-poor". Therefore, analysing the effects of income growth on poverty reduction and assessing the pro-poorness of growth are not only exercises for academic researchers but also crucial challenges for policymakers.

Prior to this, indeed, there has been an intense debate among researchers on the definition and measurement of the pro-poorness of growth, with two alternative definitions emerging: absolute versus relative. The former defines growth as pro-poor when either the absolute income gain of the poor is larger than the average income gain (strong absolute pro-poor growth) or the poor experience a positive growth rate (weak absolute pro-poor growth). The relative definition, instead, calls for the growth rate of the poorest part of the distribution to be larger than the average growth rate. Klasen (2008) highlights merits and weaknesses of each definition, arguing that the absolute (weak) definition is useful to measure the "rate" of pro-poorness, while the relative definition is particularly suitable in assessing the "state" of pro-poorness.[1]

This consideration seems consistent with the work by Ravallion and Chen (2003), who introduced the growth incidence curve (GIC). The GIC plots the percentile-specific income growth rate between two points in time. By comparing the average growth rate experienced by the individuals ranked in the poorest percentiles with the average growth rate of the overall distribution, a growth process can be defined as pro-poor in absolute (relative) terms if the former is positive (larger than the latter). In this regard, the evidence documented by Dollar et al. (2016) suggests pro-poor growth only in absolute terms, since the poorest 40 percent have experienced a positive income growth rate, without increasing their income share. Moreover, Ravallion and Chen (2003) and subsequent literature (Duclos 2009; Essama-Nssah 2005; Kraay 2006; Son 2004) measure the degree of pro-poorness of growth in an anonymous way, by focusing only on the income change experienced by each percentile of the distribution without considering the identity of individuals located on each percentile. Therefore, two alternative growth processes generating the same income distribution as the previous period are considered equivalent, irrespectively of whether individuals' positions within the income distributions are unchanged or completely reshuffled. This counterintuitive result makes the anonymous approach unsatisfactory when the intertemporal evaluation of the growth processes aims at assessing the mobility experienced by individuals. By removing the anonymity assumption, Grimm (2007) and Bourguignon (2011) propose the "non-anonymous" version of the GIC, which is obtained by keeping constant individuals' positions in the initial income distribution. Thus, the non-anonymous GIC plots the income growth rate of all individuals as a function of their quantile in the initial distribution. A growing strand of recent literature adopts this

[1] See Essama-Nssah and Lambert (2009) for a review of the literature developing indices of pro-poorness of growth. See also Duclos (2009) for a formal characterization of absolute and relative pro-poorness.

non-anonymous approach to evaluate pro-poor growth (Jenkins and Van Kerm 2016; Lo Bue and Palmisano 2020; Palmisano 2018; Palmisano and Peragine 2015).[2]

The individual income growth rate is also used by another relevant strand of literature aimed at measuring income mobility. Notably, the income mobility profiles proposed by Van Kerm (2009) represent an alternative formalization of the non-anonymous GICs.[3] However, while the analysis of mobility is quite developed (Fields 2008b; Fields and Ok 1999), the investigation of the effect of mobility on the pro-poorness of growth is still limited.[4]

Both the non-anonymous GIC and mobility profiles may offer only a partial representation of the individual income growth process; this could be the result of either shocks or measurement errors. Ferreira (2012) raised this issue in the context of the anonymous GIC, proposing an alternative interpretation based on the literature on counterfactual distributions (DiNardo et al. 1996; Juhn et al. 1993). According to Ferreira (2012), the individual income growth rate can be expressed as the sum of various components, each measuring the impact of a specific determinant, such as changes in worker characteristics or their corresponding returns. This approach has been recently applied by Ferreira et al. (2019), who estimate a counterfactual GIC to relate the distributional impact of economic growth to changes of the structure of the economy. Even Fields et al. (2015), in their analysis of earnings mobility in Argentina, Mexico, and Venezuela, recognize the confounding role of measurement errors and transitory earning shocks to which individuals may be subjected in the short-run. Therefore, they propose a framework in which individuals' earnings are decomposed as the sum of two components, one associated with observable and permanent individual characteristics and another that is related to transitory earning components.[5]

By applying the non-anonymous GIC framework, in this paper we compare actual growth episodes at each percentile of the initial personalized distribution with a counterfactual pattern of predicted income dynamics, which rules out the presence of shocks and measurement error. Comparison between the observed and the counterfactual non-anonymous GIC allows an understanding of the extent to which growth-shaped individual income trajectories have resulted from unexpected changes in the marginal return of individual socio-economic characteristics, which substantially changed individual rankings in the income distribution.

Using longitudinal survey data from Indonesia, we show that growth has been generally pro-poor over the period 2000–2014, with the incidence of growth in the initial poorest quintile being larger than expected. We apply a double selectivity model of state-dependency to better understand the nature of these unpredicted percentile-specific gains, which—as we find in this study—has evolved over time. Our results, indeed, suggests that while the economic transformations of the early 2000s contributed to improve the growth potential

[2] More specifically, Jenkins and Van Kerm (2016) and Palmisano and Peragine (2015) propose a welfare analysis of the distributive impact of growth. Palmisano (2018) suggests that the identification of individuals may be based on the ranking in the final distribution, and therefore pro-poorness is evaluated by focusing on the income trajectories of individuals who become poor. Lo Bue and Palmisano (2020) propose a non-anonymous version of GIC to evaluate the patterns of mobility experienced by the chronic and transitory poor, where identification is based either on the initial or the final distribution.

[3] The concept of mobility is multidimensional (Fields and Ok 1999), as it embodies four different aspects, which are described by Jantti and Jenkins (2014): re-ranking within the income distribution, income growth, inequality reduction, and uncertainty.

[4] In this regard, exceptions are the contributions by Bárcena and Cantó (2018) and Bresson et al. (2019).

[5] By adopting a two-stage least squares procedure, Fields et al. (2015) first estimate the part of individuals' earnings associated with permanent characteristics. Then, the predicted values, which represent a proxy of the initial income of individuals, are used in a second regression as the explanatory variable of the individuals' income changes.

at the bottom of the distribution, in the more recent years the difference between actual and expected growth merely results from individuals' ability to recover from previous negative losses, rather than from pure exogenous positive shocks.

The remainder of the paper is structured as follows. In Section 2 we characterize the counterfactual individual growth incidence curve, introduce the concept of pro-poor shock within the individual growth incidence curve framework, and present the statistical inference procedures applied. The empirical illustration is presented in Section 3, based on data from Indonesia for the period 2000–2014. Section 3.2.2 concludes.

2 Setting

2.1 The counterfactual individual growth incidence curve

Let $F\left(y_{t-1}\right)$ denote the cumulative distribution function (*cdf*) of the income observed in time $t-1$ of a population with bounded support $(0, y^{max})$ and finite mean $\mu(F) = \int_0^{y^{max}} ydF(y)$. The left inverse continuous distribution function or quantile function, showing the income of an individual occupying position $p_{t-1} \in (0, 1)$ in the distribution of incomes ranked in increasing order, is defined as $F^{-1}\left(p_{t-1}\right) := inf\left\{y_{t-1} : F\left(y_{t-1}\right) \geq p_{t-1}\right\}$ To simplify the exposition, in the remainder of the paper we equivalently denote the quantile function with $y_{t-1}\left(p_{t-1}\right)$. Likewise, $F\left(y_t\right)$ denotes the *cdf* of income observed in period t, while $y_t\left(p_{t-1}\right)$ denotes the income experienced in time t by the individual ranked p_{t-1} in period $t-1$. We rely on the non-anonymous version of the growth incidence curve (denoted hereafter as *individual* GIC, IGIC), where the identity of each individual is formalized by their rank in the initial income distribution. Following Grimm (2007), in such a setting, the income growth rate experienced by the individuals located at the p^{th} percentile in period $t-1$ can be formalized as:

$$g_t\left(p_{t-1}\right) = \frac{y_t\left(p_{t-1}\right)}{y_{t-1}\left(p_{t-1}\right)} - 1 \tag{1}$$

and, by integrating the area below the IGIC up to the initial headcount index H_{t-1}, one obtains the individual rate of pro-poor growth (IRPPG) that is:

$$IRPPG_t = \frac{1}{H_{t-1}} \int_0^{H_{t-1}} g_t\left(p_{t-1}\right) dp_{t-1} \tag{2}$$

which defines a non-anonymous pattern of growth as pro-poor if it is positive (absolute definition) or it exceeds the average growth rate measured over the entire distribution (relative definition).

At the generic time t, the observed income y of each individual can be defined as a function of a vector C of her characteristics (such as education, employment status, age, and household demographic characteristics) and a measurement error denoting her propensity to misreport income.[6] That is, the percentile-specific income dynamics can be decomposed

[6] As recently investigated by Angel et al. (2019), measurement error in reported income occurs, for example, because of the presence of a social desirability bias in survey response or specific socio-demographic characteristics of the respondents. When per capita consumption expenditure is used as a proxy for individual wealth (as in the empirical application of this paper), its misreporting is mostly related to socio-demographic characteristics of the respondents, the recall bias and the survey design.

into changes related with individual characteristics and with the individual propensity to misreport income, and variations in the income function that can be interpreted as variations of the marginal returns associated with individual characteristics.

Let $\hat{y}_t^j(p_{t-1})$ denote the income of the individual ranked in the p^{th} position in time $t-1$, predicted according to individual's attributes at the beginning of period t and the income of the previous period. Then, a counterfactual IGIC (CIGIC) can be derived to show the income that the individual located at the generic p^{th} percentile would experience in period t based on a linear prediction of her observed characteristics and ruling out the impact of economic shocks and measurement error:

$$\hat{g}_t^j(p_{t-1}) = \frac{\hat{y}_t^j(p_{t-1})}{y_{t-1}(p_{t-1})} - 1 \tag{3}$$

where the superscript j indicates that the predicted income of each individual results from two alternative regression models. Specifically, when $j = FE$ the fitted values are extracted from the following panel two-way regression model:

$$log(y_{i,t}) = \beta_0 + \beta_1 C_{i,t} + \beta_2 log(y_{i,t-1}) + \tau_t + \mu_i + \vartheta_d + u_{i,t} \tag{4}$$

where the term τ_t denotes the year dummies, the parameters μ_i and ϑ_d are the individual and the location (e.g., province of residence) fixed effects respectively, and $u_{i,t}$ represents the residual terms. By adding the prediction of the individual fixed effects to the standard fitted values, $\hat{y}_t^{FE}$ captures the effects of changes in observed individual characteristics and of unobserved time invariant characteristics.[7]

Alternatively, when $j = QR$, we let the effect of the predictors change according to the individual's rank in the final per capita (p.c. thereafter) expenditure distribution and extract the predicted values from a quantile regression that models the conditional quantiles q of the joint distribution of p.c. expenditure and its predictors as

$$Q_q log(y_{i,t}) = \beta_0(q) + \beta_1(q) C_{i,t} + \beta_2(q) log(y_{i,t-1}) + \vartheta_d + u_{i,t} \tag{5}$$

with the terms ϑ_d and $u_{i,t}$ denoting the location fixed effects and the error term respectively.[8]

To gauge the impact of economic shocks on the individual upward and downward mobility patterns, we need to compare the IGIC in Eq. 1 with the CIGIC in Eq. 3. The differential between these two curves is defined as

$$\Delta g_t = g_t(p_{t-1}) - \hat{g}_t^j(p_{t-1}) = \frac{y_t(p_{t-1}) - \hat{y}_t^j(p_{t-1})}{y_{t-1}(p_{t-1})} \tag{6}$$

This residual can be interpreted as a broad measure of the impact of the shock on the percentile-specific income growth rates. It includes, indeed, both the effect of variations of

[7] It is to be noted that, as long as the time dimension t doesn't tend to infinity, a fixed effects estimation of this dynamic linear panel equation results in a downward bias of the coefficients of interest (Nickell 1981). Alternative estimators, such as the difference GMM or the system GMM have been proposed to correct for this potential bias. However, in the context of this study (which spans a period of 14 years covered in only three waves) the use of these estimators entailed the rejection of the null hypothesis of the overall validity of the instrument set such that they could not be applied.

[8] Precisely, we estimate four quantile regressions for the $q = .20, .40, .60, .80$ conditional quantiles of the joint distribution of income and its predictors at time t, extract from each model the predicted values ($\hat{y}_t^{QR}$) and assign them to each individual depending on their position in the p.c. expenditure distribution at time t.

unobserved characteristics and their associated returns that influence individual incomes, and the effect of the error component, the role of which is discussed and tested in Section 3.2.

By using Eq. 6, we define a shock as pro-poor in absolute terms if the average of the Δg_t up to the poverty line is positive, i.e., if the positive differences between the IGIC and the CIGIC more than compensate the negative ones for all percentiles up to the poverty line. That is, an absolute index of pro-poorness of shocks can be formalized as

$$PPS_t = \frac{1}{H_{t-1}} \int_0^{H_{t-1}} \Delta g_t(p_{t-1}) dp_{t-1} \tag{7}$$

A relative definition of pro-poor shock requires that the differential defined in Eq. 6 is on average larger for the poor than for the rich. That is, let γ_t denote the average difference between the IGIC and the CIGIC over the entire distribution; then a shock is pro-poor in relative terms if $PPS_t > \gamma_t$.

2.2 State-dependency, sample rotation, and recovery from past negative shocks

When examining the role that shocks have on the mobility patterns over subsequent spells of growth, a complementary exercise is to assess the nature of the shocks themselves. For example, one could ask whether the positive shock implied in the setting characterized by $IRPPG > 0$ and $PPS > 0$ is the outcome of a genuine positive shock experienced by the initially poorer, or if it is a consequence of a recovery from past negative shocks.

To answer this question, we need to assess, from an inter-temporal perspective, whether there is some form of state-dependence, or current positive shocks are exogenous to past negative shocks. Given the definitions in Eqs. 3, 5 and 6, an individual positive shock $\left(ps_{i,t}\right)$ can be defined as a binary indicator equal to 1 if $y_t - \hat{y}_t^{QR} > 0$, and equal to 0 otherwise.

Let's start by assuming that each individual has a latent propensity to experience a positive shock in time t, and let's set the hypothesis that this is a function of a vector of individual and place-of-residence characteristics ($X_{i,t-1}$), the individual's propensity to have experienced a negative shock in the past $\left(ns^*_{i,t-1}\right)$, and to have been retained $\left(r^*_{i,t}\right)$ in the sample[9]:

$$ps^*_{i,t} = f\left(ns^*_{i,t-1}, r^*_{i,t}, X_{i,t-1}\right) \tag{8}$$

Following the approach proposed by Cappellari and Jenkins (2004; 2008), $ns^*_{i,t-1}$ can be defined as:

$$ns^*_{i,t-1} = \eta Z_{i,t-1} + \epsilon_i \tag{9}$$

where Z is a vector of socio-economic variables, including parental socio-economic background. If this propensity exceeds some unobserved value (which can be set equal to 0), a negative shock is observed:

$$ns_{i,t-1} = 1\left[ns^*_{i,t-1} > 0\right] \tag{10}$$

[9] Attrition is an issue that in our setting can arise from either sample attrition or missing per capita expenditure (in years $t-2, t-1$ and t) and/or in all the other variables used to obtain predicted per capita expenditure. If sample dropouts are not random and individuals with less favorable characteristics are also less likely to stay in the sample, our estimated transition probability of a positive shock experience in time t will be biased.

with $ns_{i,t-1}$ being the observable binary indicator, equal to 1 if $y_{t-1} - \widehat{y}_t^{QR} \leq 0$ and to 0 otherwise. The individual's chances of remaining in the sample are captured by $r^*_{i,t}$, the individual's latent propensity to be retained, which is a function of a vector W of individual and household characteristics, including the variables in Z and additional covariates on the quality of the interview:

$$r^*_{i,t} = \zeta W_{i,t-1} + \varepsilon_i \tag{11}$$

whose observed counterpart is:

$$r_{i,t} = 1\left[r^*_{i,t} > 0\right] \tag{12}$$

Following the procedures recommended and adopted in Sarkar et al. (2019), Tunali (1986) and Vella (1998), we focus on the *recovery* case (i.e., $ns_{i,t-1} = 1$ and $r_{i,t} = 1$), estimate Eqs. 9 and 11 simultaneously with a bivariate probit selection model, and extract the following two selection correction terms:

$$\lambda'_{i,t-1} = \phi\left(\eta Z_{i,t-1}\right) \frac{\Phi\left(\frac{\zeta W_{i,t-1} - \rho\eta Z_{i,t-1}}{\sqrt{1-\rho^2}}\right)}{\Phi_2\left(\eta Z_{i,t-1}, \zeta W_{i,t-1}; \rho\right)} \tag{13}$$

and

$$\lambda''_{i,t-1} = \phi\left(\zeta W_{i,t-1}\right) \frac{\Phi\left(\frac{\eta Z_{i,t-1} - \rho\zeta W_{i,t-1}}{\sqrt{1-\rho^2}}\right)}{\Phi_2\left(\eta Z_{i,t-1}, \zeta W_{i,t-1}; \rho\right)} \tag{14}$$

where $\Phi_2(.)$ is the bivariate standard normal distribution function, $\Phi(.)$ and $\Phi(.)$ are the standard normal density and cumulative distribution functions respectively, and $\rho = corr\left(\epsilon_i, \varepsilon_i\right)$. To test for the true exogeneity of positive shocks, we include the correction terms $\lambda'_{i,t-1}$ and $\lambda''_{i,t-1}$ in a linear probability model of *recovery* which estimates the probability of experiencing a positive shock in time t, conditional on negative shock experience in the past and sample retention[10]:

$$Prob\left(ps_{i,t} = 1 | ns_{i,t-1} = 1, r_{i,t} = 1\right) = \alpha X_{i,t-1}, +\beta\lambda'_{i,t-1} + \gamma\lambda''_{i,t-1} + u_{i,t} \tag{15}$$

If $\beta = \gamma = 0$, we can conclude that if a positive shock experienced at time t is observed, this cannot be identified as a recovery from negative shock in the past, nor can it be due to sample retention.

3 Empirical application

This section presents the empirical application of the approach proposed in the previous section. By using the IGICs and the corresponding CIGICs, we first illustrate the income growth process experienced in Indonesia over the period 2000–2014. We distinguish two sub-periods, 2000–2007 and 2007–2014. The pattern of IGICs and CIGICs and the differences between them

[10] The application of a linear probability model in this context facilitates the inclusion of the correction terms and the interpretation of their coefficients.

at any percentile are informative of the impact of mobility on the pro-poorness of growth and on the role of shocks and measurement error in shaping the observed mobility patterns. As argued above, our interpretation of the CIGICs essentially hinges on the assumption that individuals' propensity to under-/over- report their income is constant over time, i.e., that measurement error is classical. To validate this assumption, we test whether different convergence parameters predicted under the assumption of classical measurement error are sufficient to yield consistent estimates, thereby supporting the validity of our assumption of a time constant error term. Last, we assess the nature of the shocks by implementing the procedure described in Section 2.2.

3.1 Data

The empirical analysis relies on data from the *Indonesia Family Life Survey* (IFLS), one of the largest longitudinal developing-country survey data-sets.[11] We use three waves (2000, 2007, and 2014) and evaluate mobility patterns in terms of changes in monthly household p.c. consumption expenditure. This is a suitable proxy for household wellbeing in developing countries, where the primary source of income is from agriculture or informal sector, and it can also serve as an indicator of permanent household income (Cutler et al. 1991; Meyer and Sullivan 2003). Total household expenditure includes household expenditure on food and non-food items. Data on food consumption includes expenditure on both self-produced and purchased products. Households report detailed expenses on various food items such as staples, meat, dried fruits, and vegetables on a weekly basis. Each food expenditure was then multiplied by 4.3 to obtain the monthly expenditure. Household non-food expenditure is reported monthly and includes expenses on durables, such as appliances and furniture, as well as non-durables (less frequently purchased items), housing costs, and education expenses.[12] It also includes transfers in and out of the household. Heterogeneities in prices across time and space are taken into account by using temporal and spatial deflators with reference to Jakarta prices in 2002.[13] To construct a CIGIC we use observed p.c. expenditure in year $t-1$ and predicted p.c. expenditure in year t, which is estimated using information on p.c. expenditure in the previous wave, household socio-demographic characteristics (residence and composition by age group), and household head characteristics (gender, age, education, and employment status). For the second part of our analysis (i.e., the procedure illustrated in Section 2.2) we also use IFLS2 from 1997 (Frankenberg and Thomas 2000) to retrieve the variables that are necessary to estimate $\hat{y}_t^{QR}$ and all the explanatory variables used in columns 1 and 2 of Table 6.

3.2 Results

3.2.1 Actual and counterfactual individual growth incidence curves

Figure 1(a) and Fig. 1(b) illustrate the growth process experienced in Indonesia over the period 2000–2007 and 2007–2014 respectively, while Table 1 reports some summary statistics about these processes. The black dashed curve corresponds to the IGIC, which describes the

[11] For details on the surveys see Strauss et al. (2004) for 2000 (IFLS3); Strauss et al. (2016) for 2007 (IFLS4); Strauss et al. (2016) for 2014 (IFLS5).

[12] Education expenses are reported for the past year. They include expenditure on tuition, uniform, transportation, and boarding for children living outside the household.

[13] Data on both the consumer price index (CPI) and regional poverty lines (urban and rural) come from Indonesia's central statistics agency, Badan Pusat Statistik (BPS).

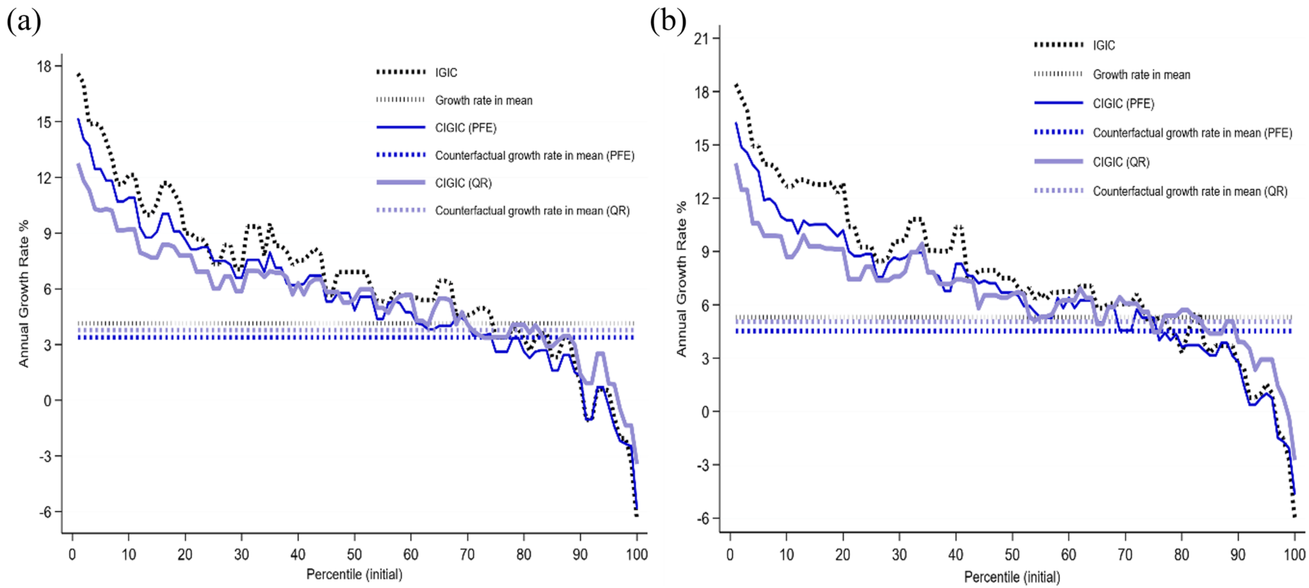

Fig. 1 IGIC and CIGICs, Indonesia sub-period 2000–2007 (**a**) and 2007–2014 (**b**).
Notes: PFE = panel fixed effect regressions; QR = quintile regressions. Source: Authors' illustration based on IFLS data

Table 1 Summary statistics of the pro-poorness growth and shocks

Panel A: Predicted values from PFE regression	2000-2007		2007-2014	
	Actual	Predicted	Actual	Predicted
Annual growth in mean	4.14	3.38	5.30	4.53
IRPPG 10	14.33	12.39	14.93	13.03
IRPPG 25	11.70	10.37	12.97	11.16
IRPPG 50	9.70	8.49	10.95	9.51
PPS 25	1.33		1.94	
PPS 50	1.23		1.53	
PPS 75-100	0.38		0.35	
Panel B: Predicted values from quintile regression	**2000-2007**		**2007-2014**	
	Actual	Predicted	Actual	Predicted
Annual growth in mean	4.14	3.78	5.30	5.05
IRPPG 10	14.33	10.47	14.93	10.89
IRPPG 25	11.70	8.84	12.97	9.68
IRPPG 50	9.70	7.51	10.95	8.54
PPS 25	2.85		3.51	
PPS 50	2.16		2.52	
PPS 75-100	-0.91		-1.34	

Source: Authors' estimations based on IFLS data

observed percentile-specific mobility patterns, while the two continuous curves are associated with the CIGICs showing the counterfactual scenario ruling out the presence of shocks and measurement error. In both subperiods the actual mobility profiles of the Indonesian populations were pro-poor, with individuals ranked below the 70th percentile experiencing an income growth rate larger than the average. The two CIGICs exhibit the same pattern as the IGIC. The counterfactual based on the panel fixed effect regression (CIGIC-PFE) tends to lie at no point above the IGIC, with almost no differences between IGIC and CIGIC for observations ranked at the 25th percentile and for those located in the top decile. The CIGIC obtained using quintile regression (CIGIC-QR), instead, is located below the CIGIC-PFE and below the IGIC up to the 40th percentile, while for the top 20 percentiles the rank between the actual and counterfactual curves is reversed, with the CIGIC-QR placed above the actual one. It can be noted, moreover, that the difference between the two CIGICs, which can be attributed to unobserved time-invariant individual characteristics that are not accounted for in the CIGIC-QR, tends to be larger for the bottom and top 20 percent of the initial distribution.

Overall, the ranking between the actual and the two counterfactual curves suggests that in both periods the consumption expenditure growth rates experienced by the individuals initially located at the bottom part of the distribution were larger than expected.[14]

[14] The ranking between the actual and each of the two CIGICs is reflected in the difference between the actual and predicted annual growth rates in mean reported in Table 1. We observe a relatively large difference when comparing the actual growth rate in mean with the predicted one based on the panel fixed effects regressions. Recall, that the expectation that the actual and predicted growth rates in mean should be the same would be fulfilled if the negative differences between actual and predicted growth for some individuals are compensated by positive actual-predicted differences for other individuals (due, for example, to different types of shocks at different part of the distribution and, taking into account measurement error, systematic under- and over-reporting of income at the top and bottom of the distribution). We observe the CGIC-PFE tends to lie below the IGIC and at the top of the distribution it overlaps with it. Therefore, the observed difference between the actual and predicted growth rate in mean is reflecting the fact that the "positive shocks" take place only in one part of the distribution.

Assuming that individuals' propensity to under-/over-report their consumption expenditure is constant over time,[15] the observed positive difference between the actual and predicted growth can be interpreted as the impact of a positive shock on the income growth rates of the initially poor. Given the definitions provided in Section 2.1, this positive shock is the gross effect of changes in individual unobserved characteristics and their associated returns, as well as economic shocks (e.g., changes in the broader structure of the economy) that favoured the income growth of the poor. The proposed measure of the shock pro-poorness, i.e., the index PPS, is indeed positive and decreasing over the income distribution (see Table 1), with the largest values associated with the period 2007–2014.

To test the significance and the heterogeneity of the growth processes we apply the Kolmogorov-Smirnov (KS) and the Cramér-von Mises (CVM) tests. This inference procedure, recently applied by Ferreira et al. (2019) in the context of anonymous GICs, checks whether: *(i)* the actual and predicted income dynamics that we observe for any initial percentile are statistically different from zero, and *(ii)* these dynamics are not significantly different along the initial distribution, i.e., IGIC and CIGIC are equal to the average growth rate for all percentiles. Table 2 reports the results for this validation exercise. As the figures suggest, the observed dynamics described by the actual and counterfactual curves are found to be statistically significant by the inference tests for the significance and the uniformity of the growth process. The KS and the CVM tests reject the null hypothesis that the observed growth process is static and distribution-neutral over the period considered, i.e., the IGIC is strongly significantly different from zero and different from the average growth for any percentile in both sub-periods. Moreover, the two tests reject the null hypothesis that in both periods our counterfactual income distributions did not change at all. When we test the distribution neutrality of the growth process, conditional on the joint distribution of covariates, both the KS and the CVM test reject in most cases the null hypothesis that the CICIGs are equal to the average growth rate at any percentile, which is consistent with the heterogeneous patterns illustrated in Fig. 1.

3.2.2 The role of measurement error

As discussed above, our interpretation of the difference between the actual and predicted percentile specific growth rates relies on the assumption that measurement error is classical. Yet, measurement error can display some non-classical features, such as being related to individual socio-economic characteristics and being serially correlated over time. Most of the literature examining the effect of measurement error on individual income growth considers earnings and income data and provides evidence that measurement error in income or earnings is serially correlated over time and negatively correlated with the true

[15] This assumption on the classical nature of the error term is discussed and tested in the next sub-section. Further error arising from the misspecification of the functional form have been tested by running alternative specifications with a quadratic per capita expenditure term. The results of this exercise produced virtually no difference between the estimated CIGIC obtained from Models 4 and 5 and the ones obtained from the augmented model with per capita expenditure squared.

Table 2 Kolmogorov–Smirnov (KS) and Cramér-von Mises (CVM) tests

	Null hypothesis	2000-2007 KS	Critical values 1%	5%	10%	2007-2014 KS	Critical values 1%	5%	10%
IGIC	$g(p)=0$	0.178	0.109	0.094	0.090	0.186	0.119	0.104	0.099
	$g(p)=\bar{g}$	0.130	0.107	0.095	0.089	0.133	0.117	0.106	0.101
CIGIC (FE)	$\hat{g}^{FE}=0$	0.160	0.084	0.077	0.072	0.166	0.084	0.073	0.068
	$\hat{g}^{FE}=\bar{g}^{FE}$	0.117	0.085	0.076	0.072	0.104	0.085	0.074	0.069
CIGIC (QR)	$\hat{g}^{QR}=0$	0.132	0.093	0.083	0.079	0.141	0.094	0.083	0.078
	$\hat{g}^{QR}=\bar{g}^{QR}$	0.090	0.093	0.083	0.079	0.089	0.095	0.083	0.079
	Null hypothesis	CVM	Critical values 1%	5%	10%	CVM	Critical values 1%	5%	10%
IGIC	$g(p)=0$	6.637	2.686	2.525	2.472	7.864	2.781	2.635	2.574
	$g(p)=\bar{g}$	3.475	2.661	2.515	2.449	3.629	2.746	2.602	2.523
CIGIC (FE)	$\hat{g}^{FE}=0$	5.867	2.019	1.912	1.870	6.737	1.988	1.858	1.801
	$\hat{g}^{FE}=\bar{g}^{FE}$	3.186	2.013	1.896	1.857	3.098	1.971	1.833	1.780
CIGIC (QR)	$\hat{g}^{QR}=0$	5.473	2.226	2.132	2.072	6.518	2.229	2.137	2.078
	$\hat{g}^{QR}=\bar{g}^{QR}$	2.395	2.224	2.119	2.054	2.244	2.221	2.116	2.064

Source: Authors' estimations based on IFLS data

values of the variable of interest (Bound and Krueger 1991; Pischke 1995), leading therefore to a bias in the estimated parameter of income (or earnings) mobility.[16]

Differently from earnings, consumption is less susceptible to measurement error as it is more easily recalled and measured (Meyer and Sullivan 2003; Aguiar and Hurst 2005). The IFLS food expenditure data is based on a short (7-day) recall questionnaire, which tend to reduce omission and telescoping error. Moreover, people are generally more willing and precise in reporting their expenditures compared to reporting their earnings and other monetary transfers (Meyer and Sullivan 2003). Yet, as shown in Gibson et al. (2015), measurement error in consumption expenditure can display some non-classical features. Typically, as households get richer and their consumption pattern gets more varied, reported expenditures might be smaller than true consumption.

The literature on the measurement of income mobility essentially proposes two approaches to address non-classical measurement error. The preferable strategy is to combine survey data and register records to estimate income or earnings mobility. However, this approach cannot be applied to our analysis. As for most developing countries, validation data for survey data or for consumption expenditure is, indeed, not available.[17] A more recent literature analysing

[16] To this regard, Antman and McKenzie (2007) show that the estimated mobility parameter from a simple OLS regression of current earnings against past earnings presents an asymptotic bias. In addition to the classic attenuation bias, the bias in the estimated mobility parameter results, indeed, from three additional sources. First, the covariance between current and past period's measurement error, which, as demonstrated in several US validation studies tends to be positive. Second, the positive covariance between the current period shocks to earnings and past reported earnings, which is attributable to the presence of individual fixed effects in the error term. Third, the covariance between true earnings and the measurement error, which as shown by Bound and Krueger (1991) tends to be negative.

[17] Moreover, it can be noted that even in contexts where administrative data is available, this approach can entail some drawbacks. The administrative and survey data matching in most studies is often conducted based on either register data or error-prone self-reported identifiers, such as social security numbers. As argued in Angel et al. (2019), given that the (voluntary) consent of individuals to match survey and register data is usually needed, the sample is biased towards individuals giving more accurate responses. Furthermore, as shown in Jenkins et al. (2006), Jenkins et al. (2008), Sakshaug et al. (2012), and Sakshaug and Eckman (2017), samples based on optional matching are often found to be non-representative for the whole population.

income mobility and poverty dynamics in developing countries contexts therefore makes use of "synthetic" or "pseudo" panels.[18] In this approach, used when genuine panels are not available, successive cross-sectional survey waves are used to track the average income for households with heads from the same birth cohort over time. As argued in Antman and McKenzie (2007), the within-cohort averaging procedure removes the effects of income measurement error in sufficiently large cohorts, even where this error is non-classical in nature. Yet, synthetic or pseudo-panels are applied when only cross-sectional data is available. A recent contribution by Moreno et al. (2021), comparing mobility indicators produced with synthetic and genuine panel data from Mexico, shows that income mobility indicators are reasonably similar for the two types of panels and are most often not statistically different.[19]

With the limitations in applicability of the two approaches mentioned above and considering the available data, we can only offer indirect evidence for the absence of mean-reverting non-classical measurement error. Building on previous work by Burger et al. (2016), we first assess the contribution of classical measurement error on several expenditure growth patterns. Then, we test whether, allowing for classical measurement error, is sufficient to produce consistent mobility estimates, thereby validating our underlying assumption on the CIGICs and providing (indirect) evidence for the absence of non-classical measurement error.

We start by defining a true p.c. consumption expenditure growth process as:

$$\Delta y_t^* = y_t^* - y_{t-1}^* = \mu + \beta y_{t-1}^* + u_t \tag{16}$$

where u_t is a stochastic consumption shock, assumed to be $iid\left(0, \sigma_u^2\right)$ (Fields 2008a).

Supposing that reported consumption expenditure, y_t, suffers from classical measurement error, i.e., $e_t \equiv y_t - y_t^* \sim iid\left(0, \sigma_e^2\right)$, Eq. (16) can be re-written as:

$$\Delta y_t = \mu + \beta y_{t-1} + u_t + e_t - (\beta + 1)e_{t-1} \tag{17}$$

where the negative correlation between initial reported consumption expenditure and the error term via the initial period measurement error term e_{t-1}, tends to cause a downward bias in the OLS estimate of the β parameter. Assuming that both e_t and u_t are *i.i.d.*, the expected value of the OLS slope coefficient (denoted as θ_1) obtained from regressing $\Delta \mathrm{y_t}$ on $\mathrm{y_{t-1}}$ can be expressed as:

$$E\left(\theta_1\right) = \frac{Cov\left(y_t, y_{t-1}\right)}{Var\left(y_{t-1}\right)} = \frac{\beta Var\left(y_{t-1}\right) - (\beta + 1)\sigma_e^2}{Var\left(y_{t-1}\right)} = (\beta + 1)\alpha - 1 \tag{18}$$

[18] See, among others, Antman and McKenzie (2007), Cuesta et al. (2011), Dang et al. (2014) and Dang and Lanjouw (2018).

[19] It has been shown, moreover, that the procedures outlined in pseudo-panels studies are not exempt from limitations and critiques. Indeed, the within-cohort averaging procedure, by definition, eliminates all the within-cohort variation in household expenditure or income, resulting in low accuracy and making the estimates highly vulnerable to any deviations from its identifying assumptions. For instance, Fields and Viollaz (2013), applying pseudo-panels estimators to true panel data show that the synthetic panels method do not accurately estimate actual income mobility and conditional poverty dynamics. Similarly, Hérault and Jenkins (2019), using Australian and British data, demonstrate that the validity of estimates of poverty dynamics statistics produced with synthetic panel approaches as in Dang and Lanjouw (2018), can suffer from low accuracy, as it crucially depends on choices related, for instance, to the age of the household head defining the sample, the poverty line level, and the years analyzed.

where the parameter α represents the share of total variation in initial p.c. expenditure that is due to the variation of true initial p.c. expenditure y^*_{t-1}, rather than measurement error e_{t-1}:

$$\alpha \equiv \frac{Var\left(y^*_{t-1}\right)}{Var\left(y_{t-1}\right)} = \frac{Var\left(y^*_{t-1}\right)}{Var\left(y^*_{t-1}\right) + \sigma^2_e} \tag{19}$$

This parameter which informs us on the reliability of the reported initial value of p.c. expenditure,[20] lies in the interval $[0, 1]$. It follows from Eq. (18) that a value of 1 points to the absence of measurement error, i.e., $E\left(\theta_1 | \alpha = 1\right) = \beta$, which gives an unbiased estimate of the OLS mobility parameter. Instead, a value of α smaller than 1 signals the presence of noise in reported p.c. expenditure, i.e., $E\left(\theta_1 | \alpha < 1\right) < \beta$.

In a data setting featuring more than two periods of observations and keeping the assumption that measurement error is classical, i.e., $e_t \equiv \Delta y^*_t \sim nid\left(0, \sigma^2_e\right)$, Eq. (16) can be estimated as:

$$\Delta y^*_t = \mu_t + \beta_t y^*_{t-1} + u_t \tag{20}$$

We further assume that β is constant over the period under consideration, i.e., $\beta_t = \beta < 0$, and that the intercept term is completely unrestricted over time, which allows our variable of interest to follow a potentially non-linear time trend represented by the parameter u_t. Maintaining these assumptions, in our three-waves panel data, several mobility coefficients (denoted as $\hat{\theta}_k$) can be estimated from several OLS regressions. First, we regress initial p.c. consumption expenditure of individual i against her absolute growth rate between two consecutive waves:

$$\Delta y_{i,2000-2007} = \mu_i + \theta_1 y_{i,2000} + u_i \tag{21}$$

and

$$\Delta y_{i,2007-2014} = \mu_i + \theta_2 y_{i,2007} + u_i \tag{22}$$

As indicated by Eq. (18), measurement error will tend to bias the estimates of $\hat{\theta}_1$ and $\hat{\theta}_2$ away from β and towards -1.

Second, we regress the absolute growth rates between the second and the last wave and between the first and the last wave on p.c. consumption expenditure in the first wave:

$$\Delta y_{i,2007-2014} = \mu_i + \theta_3 y_{i,2000} + u_i \tag{23}$$

and

$$\Delta y_{i,2000-2014} = \mu_i + \theta_4 y_{i,2000} + u_i \tag{24}$$

In the absence of classical measurement error, we expect a proportional reduction of the effect of initial consumption over time. A stationary $AR(1)$ process that eliminates in expectation $-\beta$ of the gaps in p.c. consumption expenditure between the first two waves

[20] This parameter is, indeed, known in the literature as the "reliability statistics" (Abowd and Stinson 2013; Gottschalk and Huynh 2010).

should eliminate a smaller proportion $-\beta(\beta+1)$ of the initial expenditure gap between the second and the third wave. Hence, between the first and the last wave the total proportional convergence parameter should be $-\beta(\beta+2)$. If, instead, we allow for measurement error, Eq. (18) yields that $E\left(\theta_3|\beta,\alpha\right)=\alpha\beta(\beta+1)$ and $E\left(\theta_4|\beta,\alpha\right)=\alpha(\beta+1)^2-1$.

Third, we extend Model (23) as follows:

$$\Delta y_{i,2007-2014}=\mu_i+\theta_5 y_{i,2000}+\theta_6 y_{i,2007}+u_i \tag{25}$$

Under the hypothesis of no measurement error we expect that, once we control for y_{2007}, there is a null relationship between consumption expenditure in 2000 and its change between 2007 and 2014 $\left(E\left(\theta_5|\beta,\alpha=1\right)=0\right)$ while the expected value of $\widehat{\theta}_6$ will be simply the convergence parameter β. If we allow for measurement error, Eq. (18) yields that $E\left(\theta_5|\beta,\alpha\right)=\frac{(\beta+1)^2(\alpha-1)\alpha}{\alpha^2(\beta+1)^2-1}$ and $E\left(\theta_6|\beta,\alpha\right)=\frac{1-\alpha(\beta+1)+\alpha^2\beta(\beta+1)^2}{\alpha^2(\beta+1)^2-1}$ implying that measurement error exacerbates the downward bias in the coefficient of y_{2007} and causes an upward bias on the coefficient of y_{2000}.

Lastly, we regress the absolute change in p.c. consumption expenditure between the last two waves on its change between the first two waves:

$$\Delta y_{i,2007-2014}=\mu_i+\theta_7\Delta y_{i,2000-2007}+u_i \tag{26}$$

In the absence of measurement error, we expect that households that experienced a larger growth in p.c. consumption expenditure between the first two waves will experience a slower subsequent growth, i.e., $E\left(\theta_7|\beta,\alpha=1\right)=\frac{1}{2}\beta$. If the data is measured with classical error, Eq. (18) yields that: $E\left(\theta_7|\beta,\alpha\right)=-\frac{1-\alpha+\alpha\beta^2}{2(1-\alpha-\alpha\beta)}$. That is, the negative correlation between the two subsequent changes in p.c. consumption expenditure should be larger than expected in the no-measurement error case. The estimated $\widehat{\theta}_k$ coefficients from Models 21–26 can be used in three distinct approaches that complement each other to test for the presence of classical measurement error.

The first approach is to directly test the hypothesis that consumption expenditure is measured without classical error (i.e., $\alpha=1$). Estimates of α can be produced by using estimates of $\widehat{\theta}_1$ and $\widehat{\theta}_3$, as:

$$\alpha=\frac{\left(\widehat{\theta}_1+1\right)^2}{\widehat{\theta}_1+\widehat{\theta}_3+1} \tag{27}$$

As reported in Table 3, the reliability statistics α in our sample is 0.64, pointing to the presence of classical measurement error.

A second approach to test for the presence of classical measurement error is to use all of the estimated regression coefficients from Models 21–26 and compare their actual values with their expected values under the two alternative scenarios that consumption expenditure is measured without or with classical measurement error. In the first scenario, the expected values of these coefficients are estimated assuming that $\alpha=1$ and that the regression coefficient $\widehat{\theta}_1$ represents the true convergence parameter β. Alternatively, the second scenario uses the reliability statistics produced by the data, as in Eq. (27), and $E\left(\widehat{\theta}_k|\beta=\frac{\widehat{\theta}_3}{\widehat{\theta}_1+1}\right)$.

If consumption expenditure data is measured without error and the assumed condition of a first-order autoregressive process is valid, then the estimated coefficient values should only differ due to sampling variation from the predicted ones under the hypothesis of classical measurement error. However, violation of these assumptions may cause significant differences between the estimated regression coefficients and the predicted values.[21]

As reported in Table 3, apart from $\widehat{\theta}_2$, the estimated regression coefficients are very different from the values predicted under the assumption of no measurement error. For instance, the effect of y_{2000} on $\Delta y_{2007-2014}$ (i.e., coefficient $\widehat{\theta}_3$) and $\Delta y_{2000-2014}$ (i.e., coefficient $\widehat{\theta}_4$) is respectively one-third and approximately 80 percent of the effect that we would have expected under the assumption of no measurement error. Analogously, we observe that measurement error exacerbates by approximately 70 percent the negative correlation $\left(\widehat{\theta}_7\right)$ between the two subsequent changes in p.c. consumption expenditure and produces a bias in the coefficients $\widehat{\theta}_5$ and $\widehat{\theta}_6$. On the other hand, the estimated $\widehat{\theta}_k$ coefficients are similar to the values reported in the third line of Table 3, which are predicted under the assumption of classical measurement error.

A third approach to provide evidence in support or against the hypothesis of classical measurement error is to compare the convergence parameters β implied by each of the estimated $\widehat{\theta}_k$ coefficients under the two alternative scenarios of no measurement error ($\alpha = 1$) and that data is measured with error, i.e., $E\left(\widehat{\theta}_k|\beta, \alpha = \widehat{\alpha}\right)$. We observe, in the fourth and fifth raw of Table 3, that the derived estimates of β under this latter hypothesis lie within a relatively narrow range, whereas those obtained under the assumption of no measurement error do not. This result indicates that our estimates obtained under the assumption of classical measurement error are enough to produce consistent estimates, providing therefore also support for the absence of non-classical measurement error.

Interestingly, this finding aligns with our results from a validation exercise proposed by Fields et al. (2003) to test if the actual expenditure dynamics simply result from mean-reverting non-classical measurement error, generating a spurious relation between the base-year reported expenditure and the associated change. The test considers the ratio of the minimum amount of variance of stochastic measurement error relative to variance of true income that would be required to overturn the observed pattern of convergence. If this ratio is large enough to exceed a critical threshold, the downward pattern of our estimated IGICs can be evaluated as robust against the hypothesis of regression to the mean. The test, which is conducted for different combinations of the serial correlation coefficients and of the correlation between base-year expenditure and measurement error, is reported in Table 4. Results suggest that the estimated negative slope of the IGIC is robust against non-classical measurement error in both periods, with the ratios largely exceeding the minimum critical threshold of 0.3 across most of the combinations of the serial correlation coefficient and of the correlation between base-year expenditure and measurement error.[22]

[21] Such discrepancies can also occur if the other underlying assumptions are not valid. For instance, the assumption of a constant slope might be problematic over a relatively long time span as the one considered in this analysis. Yet, as reported in Table A1 in the Appendix, the coefficients on initial p.c. consumption expenditure have a similar magnitude (i.e., -0.46 and -0.45) in the two regressions with dependent variables defined by changes in p.c. consumption expenditure between the first and second wave and between the second and third wave.

[22] By relying on two validation studies based on U.S. data, Fields et al. (2003) assume that a credible range for the minimum critical threshold of this ratio is equal to about 0.1 to 0.3.

Table 3 Regression coefficients and implied parameter values

θ_k	θ_1	θ_2	θ_3	θ_4	θ_5	θ_6	θ_7
Estimated values of θ_k	-0.459*** (0.007)	-0.448*** (0.007)	-0.086*** (0.008)	-0.546*** (0.007)	-0.221*** (0.008)	0.570*** (0.008)	-0.392*** (0.007)
Predicted values of θ_k under the assumption of no measurement error ($\beta = -0.459, \alpha = 1$)	-0.459	-0.459	-0.248	-0.707	0.000	-0.459	-0.229
Predicted values of θ_k under the assumption of classical measurement error ($\beta = -0.158, \alpha = 0.643$)	-0.459	-0.459	-0.086	-0.545	0.229	-0.583	-0.406
Values of β implied by $\theta_k (if\, \alpha = 1)$	-0.459	-0.448	-0.095	-0.326	na$^+$	-0.570	-0.784
Values of β implied by $\theta_k (if\, \alpha = 0.643)$	-0.159	-0.142	-0.159	-0.160	-0.170	CN	-0.208

The full regressions results on the estimation of the coefficients in the first raw are shown in Table A1 in the Appendix. Significance levels: $* \, p < 0.10$;$** \, p < 0.05$; $*** \, p < 0.01$. $^+$ Not available given that $E(\theta_5 | \beta, \alpha = 1) = 0$.

Source: Authors' estimations based on IFLS data

Table 4 Ratio of measurement error variance to true expenditure variance implying zero correlation between true initial expenditure and true change in expenditure

Correlation between base-year expenditure and measurement error	Serial correlation coefficient	2000–2007 $\beta = -0.459$	2007–2014 $\beta = -0.448$
0	0	0.848	0.812
0	0.1	1.041	0.991
0	0.2	1.346	1.273
-0.1	0	0.687	0.657
-0.1	0.1	0.843	0.803
-0.1	0.2	1.090	1.031
-0.2	0	0.543	0.519
-0.2	0.1	0.666	0.634
-0.2	0.2	0.861	0.815
-0.4	0	0.305	0.292
-0.4	0.1	0.375	0.357
-0.4	0.2	0.485	0.458

Source: Authors' estimations based on IFLS data

3.2.3 The nature of the shocks

As implied in our results so far, in both periods expenditure growth was generally pro-poor. The negatively sloped IGIC matches with the expectations on what the relative gains at each percentile should be, given individual socio-economic attributes and the returns associated with them. Nevertheless, a sizeable portion of this progressive pattern cannot be fully accounted for by this, as the actual growth rates of the poor are significantly larger than the predicted ones. We need, therefore, to understand if this "unexpected" positive growth for the poor resulted from events that do not relate to individual exposure to negative shocks in the past (e.g., changes in the labour market that increased the returns to education), or if the unpredicted income dynamics simply reflect individuals' recovery from past negative shocks, due for example to improvements in their ability to cope with negative shocks in the past, or simply the dissipation of a past negative shock. To shed light on this question, we consider for each of the two growth spells (2000–2007 and 2007–2014) the proportion of individuals that, at the end of each period experienced a positive shock $\left(y_t - \hat{y}_t^{QR} > 0\right)$, conditional on retention and on observing, at the beginning of the period, a negative income shock $\left(y_{t-1} - \hat{y}_{t-1}^{QR} \leq 0\right)$. These individuals amount to about 24 percent of the observations retained in the panel and to about 13 percent of the entire sample (see Table 5).

Attrition at time t arises from either sample attrition or missing per capita expenditure and/or in all the other variables used to obtain predicted per capita expenditure. Because individual shock experience is measured based on the household-level expenditure variable, the covariates used in the double selectivity regression model are also measured at the household level. Precisely, the covariates refer to the household head and his/her spouse (age, sex, employment status, education), and to the household itself (several variables summarizing household composition and parental socio-economic background). The standard errors are bootstrapped and estimated to be robust to heteroskedasticity and arbitrary serial correlation among observations in the same province.

Table 5 State dependency and initial shock experience with and without non-retained sample

Panel A: 2000-2007			
		Status at time t	
Status at time $t-1$	$y_t > \hat{y}_t^{QR}$	$y_t \leq \hat{y}_t^{QR}$	*not retained*
Sample retained			
$y_{t-1} > \hat{y}_{t-1}^{QR}$	28.41	24.26	
$y_{t-1} \leq \hat{y}_{t-1}^{QR}$	24.56	22.77	
All	52.97	47.03	
all individuals			
$y_{t-1} > \hat{y}_{t-1}^{QR}$	15.99	13.65	22.75
$y_{t-1} \leq \hat{y}_{t-1}^{QR}$	13.82	12.81	20.98
All	29.81	26.46	43.73
Panel B: 2007-2014			
		Status at time t	
Status at time $t-1$	$y_t > \hat{y}_t^{QR}$	$y_t \leq \hat{y}_t^{QR}$	*not retained*
Sample retained			
$y_{t-1} > \hat{y}_{t-1}^{QR}$	28.10	24.87	
$y_{t-1} \leq \hat{y}_{t-1}^{QR}$	24.33	22.70	
All	52.43	47.57	
all individuals			
$y_{t-1} > \hat{y}_{t-1}^{QR}$	14.91	13.20	24.35
$y_{t-1} \leq \hat{y}_{t-1}^{QR}$	12.91	12.05	22.58
All	27.83	25.25	46.93

Pooled transitions from IFLS, waves 2–5. Sample size (retained) = 15,960. Retained individuals are followed in 1997–2000-2007–2014 and with non-missing variables on per capita expenditure and its predictors in each year. Total sample size in Panel (A): 28,364. Total sample size in Panel (B): 30,073. Panel (A) includes individuals retained plus individuals with non-missing per capita expenditure in 1997 and 2000 and complete information on the predictors of per capita expenditure. Panel (B) includes individuals retained plus individuals with non-missing per capita expenditure in 2000 and 2007 and complete information on the predictors of per capita expenditure.

Source: Authors' estimations based on IFLS data

As implied by the estimated correlation term between negative shock experience at the baseline and retention (Table 6), those retained in the sample are more likely to experience a negative shock at the beginning of the first period. However, for the next period we do not find statistically significant evidence of initial-conditions selectivity of sample attrition. We also observe that in both periods, individuals with a lower socio-economic background are more likely to be retained in the sample. These are individuals from households in which the household head's spouse is an unpaid worker and has low education levels, with lower socio-economic background associated with their family of origin.

However, we see also that – apart from this common trend – the household demographic drivers of sample retention change substantially from one period to the other. Specifically, smaller households and households with family members above the age of 16 are more likely to drop out of the sample in the first period but more likely to be retained in the second period.

When looking at the drivers of negative shock experience at the baseline, our results suggest that lower socio-economic background of the family of origin (as proxied by years of education of the father of the household head) increases the likelihood of a negative shock. However, current socio-economic characteristics of the household head and of his spouse, such as the level of education and a job as government worker are, especially in the

Table 6 Probability of retention and initial negative shock experience – marginal effects of explanatory variables

	Retention	Initial status:$ns_{i,2000} = 1$	Retention	Initial status:$ns_{i,2007} = 1$
	(1)	(2)	(3)	(4)
Age (years) of HH head	0.008***	0.009***	0.008***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Age squared of HH head	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Female headed HH (dummy)	-0.016	-0.046***	-0.000	0.009
	(0.012)	(0.017)	(0.011)	(0.013)
Years of schooling HH head	-0.000	0.010***	-0.002*	0.028***
	(0.001)	(0.001)	(0.001)	(0.001)
Years of schooling HH spouse	-0.003*	-0.003	-0.007***	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
HH size	0.009	-0.098***	0.015*	-0.081***
	(0.009)	(0.012)	(0.008)	(0.013)
HH size squared	-0.001	0.005***	-0.001**	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Ratio of family members aged 19 +	-0.166***	0.263***	0.088**	0.317***
	(0.058)	(0.065)	(0.041)	(0.038)
Ratio of family members aged 16–18	-0.166***	0.225***	0.235***	0.342***
	(0.044)	(0.065)	(0.047)	(0.056)
Ratio of family members aged 13–15	-0.018	0.175***	0.275***	0.269***
	(0.069)	(0.066)	(0.052)	(0.036)
Ratio of family members aged 6–12	-0.069	0.050	-0.298***	0.085**
	(0.072)	(0.055)	(0.045)	(0.036)
HH head is government worker (dummy)	0.005	0.158***	-0.040	0.127
	(0.017)	(0.018)	(0.069)	(0.083)
HH head is private worker (dummy)	0.039***	-0.049***	0.012	-0.024
	(0.012)	(0.017)	(0.038)	(0.060)
HH head is unpaid worker (dummy)	0.000	0.024	0.131	-0.153
	(0.029)	(0.054)	(0.155)	(0.173)
HH spouse is government worker (dummy)	-0.017	0.151***	-0.317**	1.765***
	(0.029)	(0.036)	(0.129)	(0.053)

Table 6 (continued)

	Retention	Initial status: $ns_{i,2000} = 1$	Retention	Initial status: $ns_{i,2007} = 1$
	(1)	(2)	(3)	(4)
HH spouse is private worker (dummy)	0.035**	-0.104***	0.156	-0.021
	(0.014)	(0.027)	(0.136)	(0.262)
HH spouse is unpaid worker (dummy)	0.048***	-0.065***	0.174***	0.267**
	(0.014)	(0.018)	(0.068)	(0.105)
Parental SES: Mother's education	-0.008***	0.001	-0.006***	-0.004**
	(0.002)	(0.001)	(0.001)	(0.002)
Parental SES: Father's education	0.004**	-0.006***	-0.002	-0.003**
	(0.002)	(0.002)	(0.002)	(0.001)
Parental SES: Mother is retired (dummy)	-0.018	-0.002	0.016	-0.001
	(0.013)	(0.014)	(0.011)	(0.014)
Parental SES: Father is retired (dummy)	0.005	-0.010	-0.013	-0.016
	(0.013)	(0.011)	(0.010)	(0.010)
Parental SES: Mother is unemployed (dummy)	-0.046***	-0.004	0.003	-0.003
	(0.010)	(0.009)	(0.010)	(0.009)
Parental SES: Father is unemployed (dummy)	0.003	-0.005	-0.010	-0.010
	(0.013)	(0.010)	(0.010)	(0.009)
Accuracy of the interview (dummy)	0.047		0.121	
	(0.030)		(0.103)	
Rating of the interview missing (dummy)	0.018		-0.108***	
	(0.051)		(0.028)	
Seriousness of the interview (dummy)	0.070*		0.028	
	(0.038)		(0.091)	
Correlation between unobservable factor affecting $r_{i,t}$ and $ns_{i,t-1}$	0.037***		-0.014	
Observations	28,345	28,345	30,026	30,026
Province fixed effects	yes	yes	yes	Yes

HH = household; SES = socio-economic status. Robust standard errors clustered at the province level in parenthesis; Significance levels: $* \ p < 0.10$; $** \ p < 0.05$; $*** \ p < 0.01$ For each sample, the estimated coefficients are obtained from a bivariate probit model that jointly estimates the probability of initial status and retention, following the double selectivity model. Omitted category for the employment status of HH head and HH head's spouse is self-employment

Source: Authors' estimations based on IFLS data

Table 7 Probability of experiencing a positive shock, conditional on past negative shock and retention

	Recovery in 2007 (1)	Recovery in 2014 (2)
Age (years) of HH head	-0.010*** (0.003)	-0.010*** (0.003)
Age squared of HH head	0.000*** (0.000)	0.000*** (0.000)
Female headed HH (dummy)	-0.024* (0.015)	-0.017 (0.013)
Years of schooling HH head	-0.012*** (0.002)	-0.011** (0.006)
Years of schooling HH spouse	-0.008*** (0.001)	-0.004*** (0.002)
HH size	0.104*** (0.016)	0.119*** (0.017)
HH size squared	-0.005*** (0.001)	-0.006*** (0.001)
Ratio of family members aged 19 +	-0.241*** (0.057)	-0.285*** (0.068)
Ratio of family members aged 16–18	-0.192*** (0.056)	-0.276*** (0.047)
Ratio of family members aged 13–15	-0.226*** (0.051)	-0.097 (0.064)
Ratio of family members aged 6–12	-0.256*** (0.042)	-0.213*** (0.059)
HH head is government worker (dummy)	-0.050* (0.027)	-0.124 (0.106)
HH head is private worker (dummy)	-0.032** (0.013)	0.019 (0.085)
HH head is unpaid worker (dummy)	0.030 (0.029)	-0.338** (0.152)
HH spouse is government worker (dummy)	-0.004 (0.028)	-0.358*** (0.072)
HH spouse is private worker (dummy)	-0.034 (0.022)	0.029 (0.159)
HH spouse is unpaid worker (dummy)	-0.030** (0.015)	-0.002 (0.171)
Selection – Retention	-0.051 (0.052)	-0.070*** (0.024)
Selection – Negative shock at time $t-1$	0.109 (0.089)	0.016 (0.094)
Wald Test $\beta=\gamma=0$	2.35	14.04***
p-value	0.309	0.001
Observations	15,955	15,960
R-squared	0.100	0.097
Province fixed effects	yes	yes

Bootstrapped standard error in parenthesis. Significance levels:$*$ $p < 0.10$;$**$ $p < 0.05$;$***$ $p < 0.01$.Omitted category for the employment status of HH head and HH head's spouse is self-employment. Constant not reported.

Source: Authors' estimations based on IFLS data

first period, significantly and positively related to the experience of a negative shock at the baseline. Turning to the individual probability of recovery (i.e., positive shock experience, conditional on previous negative shock experience), we observe in Table 7 that this tends to be instead higher for individuals with a lower socio-economic background. The coefficients on the selection correction term on retention $\left(\lambda''_{i,t-1}\right)$ is negative in both periods but only statistically significant for the probability of recovery in 2014. In this latter period, the exogeneity test of initial conditions ($\beta = \gamma = 0$) strongly rejects the hypothesis that $\lambda'_{i,t-1}$ and $\lambda''_{i,t-1}$ are jointly zero, suggesting that the positive shocks observed between 2007 and 2014 are driven by sample retention and are likely to be identified as a simple dissipation of previous negative shocks.

On the other hand, the coefficients of the selection correction terms on retention and on initial negative shock experience are jointly and individually not significant in the first period. The lack of significance of the sign of the selection terms on initial negative shock experience implies that the unobserved factors that raised the probability of experiencing a negative shock at the baseline did not play a role in influencing the chances of a positive shock at the end of the period. This result can be interpreted in light of the various economic transformations experienced in Indonesia during those years, such as the perpetuation of the effects of the 1997/98 economic crises[23] or the early 2000s oil price shocks (resulting in our model as un-predicted reductions in consumption expenditure) and the economic changes experienced by the country in the Reformasi era. In the 2000s, partly as a consequence of global market trends and a natural-resource export boom, Indonesia experienced high rates of economic growth and rapid structural change which affected the composition of labour demand. Notably, employment rose mainly in the service and in low-skill sectors (Coxhead and Shrestha 2016). Whereas real labour earnings stagnated, the new employment opportunities absorbed a large share of low-skilled workers and, as found in several studies (e.g., Suryahadi et al. 2012; Suryadarma et al. 2013), contributed substantially to poverty reduction. As implied by our findings, this type of exogenous economic shocks, indeed, played a role in generating growth opportunities at the bottom of the distribution.

4 Concluding remarks

Growth incidence curves are the main tool proposed to assess the distributive impact of growth. However, this tool is unsatisfactory for a deeper investigation of the nature of the observed growth pattern, which can mask either measurement errors or the presence of shocks affecting percentiles in different ways.

This paper offers a guide to correctly interpreting the pro-poorness and mobility implications of growth processes within the context of the IGIC framework. As a first step, we compare the actual growth episodes at each percentile of the initial personalized distribution with a counterfactual pattern of income growth predicted on the basis of individual attributes. As a second step, we examine the difference between actual and counterfactual individual growth rates. This allows us to understand whether unpredicted positive growth for the initially poor is the result of genuine positive shocks, favouring upward mobility,

[23] As shown in Ravallion and Lokshin (2007), the 1997/98 crisis had an appreciable long-term impact on mean consumption and on the incidence of poverty. Precisely, almost a one-quarter drop in consumption and at least half of the observed poverty count in 2002 was attributable to the crisis.

or whether it can be attributed to processes of state-dependence and so to individual ability to recover from previous negative shocks.

The methodological framework is applied in the context of a sample of 15,960 individuals from Indonesia followed over two seven-year periods, 2000–2007 and 2007–2014. Our results document that there has been substantial and significant upward mobility among the initially poorer. However, a significant part of this progressive growth cannot be reconciled with either unobserved individual endowments or changes in certain socio-economic attributes. The main factor driving the difference between actual and counterfactual growth rate is the recovery from previous negative shocks in recent years, as well as more genuine economic shocks in the early 2000s. For Indonesia, the entire period considered in this paper has been one of rapid and sharp changes in the economy and in society. The year 2000 marks the transition from the autocratic rule of Suharto, the recovery from the Asian financial crisis, the beginning of a process of decentralization, and, subsequently, the commodity boom – four different economic, political, and social events that arguably had an impact on people's lives and so on their income trajectories. Several studies (e.g., Bresson et al. 2017; Grimm 2007; Lo Bue and Palmisano 2020), including the present one, have shown that there has been growth in this period and that the incidence of growth has been larger among the initially poor. But why do the poor exhibit higher growth rates than those individuals initially belonging to richer percentiles? The findings of this study suggest that the rapid economic transformations of the early 2000s played a role in shaping the growth potential at the bottom of the distribution. Conversely, in line with the snapshot of rising inequality and falling poverty depicted by the World Bank (2016), our results also imply that what is observed in the more recent years is the product of the coexistence of high vulnerability and reactivity to shocks for the poor and of economic security for the middle and upper-middle class that continued to grow according to expectations. We do observe high mobility among the bottom 30 per cent, but this has to be interpreted simply as resilience and ability to escape chronic poverty, rather than as a signal of increased opportunities to climb the socio-economic ladder.

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Declarations

Conflict of interest None.

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